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intensity of the linkage in the material at his disposal to be equivalent to 26.7 per cent. of crossing over.

(*b*) It has been shown directly, by means of crosses between colorless individuals in a linkage family and aleurone testers and indirectly by means of aleurone tests with a non-linkage family where the *A* factor and not the *C* factor is heterozygous, that the *C* factor for aleurone is linked with the factor for waxy endosperm.

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INHERITANCE IN ORTHOPTERA

In a recent paper (Nabours, '17) Nabours has continued his admirable studies of inheritance in *Paratettix*. The paper is backed up with an abundance of data, from which a number of facts are deduced. In his discussion, he attacks certain modern hypotheses, and since it appears to me that his strictures are not entirely justified, I venture here to review the evidence, and make certain comments on it.

The following facts, several of which were well brought out in a previous paper (Nabours, '14), are presented:

1. A large number of distinct, true-breeding forms of *Paratettix* occur "in nature." Of these he has collected at least fourteen or fifteen. He no longer looks upon each as a distinct species, and he has dropped the "specific names" he suggested for them in the previous paper.

2. The distinguishing characteristics of these forms fall into two groups in their mode of inheritance: (*a*) Fourteen color patterns act as allelomorphs to each other. (*b*) A fifteenth pattern is "allelomorphic only to its absence."

3. One of the characters of the "multiple allelomorph" group does not always act as an allelomorph to the other members of the group. This is the character I of his first paper, which was noted then for the same behavior. Rather, it behaves (to put it briefly, but in words very different from Nabours's) as if it were closely but not completely linked to the others.

Because I wrote a review of Nabours's first paper on this subject (Dexter, '14), I feel a certain responsibility for what I think are mistaken viewpoints concerning the multiple-allelomorph-nature of this group.

Bellamy, working in the same laboratory, contributes also an excellent study on the same subject, but based on a different genus of *Tetrigincæ* (Bellamy, '17). Both he and Nabours have apparently accepted as proven that there is here a large group of determiners allelomorphic to each other, and I am quoted in support of this idea. Inasmuch, however, as I made certain reservations which are important in the light of the new data presented, I beg to quote a portion of my former paper.

> As Sturtevant has pointed out, for any case to which the idea of multiple allelomorphism is applicable, an equally valid explanation may be found in "complete linkage" of the factors concerned. To decide in any case between the two explanations would be impossible. If however linkage were not complete, a "cross-over" class . . . might occur, and this would suffice to rule out the explanation based on multiple allelomorphs. Such a cross-over class perhaps is furnished by the BEI individual.

I then suggested that since the BEI individual had been lost before it could be tested, the cross be repeated, and said:

> If then BEI forms should occur again, and in these, when mated to other forms, the factors B and I should be found to stay together to the same extent as before they separated, it would show that close linkage rather than multiple allelomorphism explains this particular instance.

Nabours has repeated this experiment, using the character S instead of E, and has again obtained such a cross-over, BIS. With this individual, he has carried out breeding tests.

Apparently forgetting what had been pointed out in my paper, he says:

> The significant feature is the complete combination or linkage, apparently permanent, of the factor for S, and the factor for the modified I. . . . This combination, IS, becomes a new form, *a new multiple allelomorph* (italics not original), pairing with and allelomorphic to any other multiple allelomorph with which it has been tried. . . . It is not possible for me to suggest the means by which the combination or linkage was effected.

(One must protest against the use of words which permits a single determiner to be called a "multiple allelomorph.")

The answer that he was unable to give is obvious. Perhaps there are thirteen characters here whose determiners are allelomorphic to each other. That is possible, perhaps probable, though unproved. But I is not a member of that group, but is only linked to it, being, as we may say, in the same chromosome. The work that Nabours has done makes that certain, and disposes also, by the way, of the likelihood that non-disjunction explains the similar case in the first paper (Bridges, '16).

Nabours has made a sort of mystery of the character called G in his first paper, but now called *θ*, which, he says, is "only allelomorphic to its absence." Ignoring the philosophy of this statement, he has shown that *θ* mendelizes independently of the other characters. He suggests that such determiners may be of frequent occurrence. He has shown that by substituting Greek letters for English letters the formulæ will work out as well as they did before, and has naïvely applied the method to the case of comb inheritance in poultry. His difficulty is simply caused, and Bellamy has pointed out its solution:

> It need only be assumed that the determiner is borne by some other chromosome.

In *Drosophila* some four or five years ago, the determiner for bent wings was the only one known for the fourth chromosome group. If at that time we had known only one other set of characters in *Drosophila,* viz., that of the white-eosin group, the situation would have been parallel to the one described by Nabours. We might speak of a half dozen or so of "characters allelomorphic to each other," and of one, bent, "allelomorphic only to its absence." Later on, when we found other characters whose determiners were located in the fourth chromosome, we should modify our theory. Nabours's industry in his research makes me feel safe in prophesying that he will yet discover something linked to *θ*.

He says parenthetically that two other characters "apparently of the nature of *θ*" have been discovered. It is important to find out their linkage relations and we shall wait eagerly to hear of them. In the meantime we must conclude that he has discovered the beginning of at least two chromosome groups.

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